

regard the use of the terms "sporangium," "macrospore," "microspore" as unnecessary, seeing that the book deals only with the flowering plant; and why "oosphere" and not "ovum"?

In the second part short chapters explain the Linnæan and natural systems of classification, the distribution of plants, and give general directions for field work.

The physiology is the least satisfactory part of the book. The plan adopted of giving experiment, result and conclusions to be deduced therefrom is eminently good. But many of the experiments are open to serious criticism, as in some cases the apparatus is not practical, in others the deductions are unsatisfactory. For instance, apparatus is figured on p. 126 to show that plants take in oxygen. The apparatus shown would certainly allow leakage of air; the potash would not absorb much carbon dioxide, and in so far as it did, this would partly account for the change in the manometer; further, the seedlings shown in the figure would photosynthesise unless placed in the dark. The three subsequent figures also show apparatus which is not workable. Despite these faults and one or two erroneous statements, the book is so vigorous and well compounded that it may be strongly recommended to school teachers as one which is eminently suitable for beginners in botany.

Intuitive Suggestion. By J. W. Thomas. Pp. x + 160. (London: Longmans, Green and Co., 1901.) Price 3s. 6d. net.

IT is difficult to know how far Mr. Thomas takes himself seriously. His book is called a "New Theory of the Evolution of Mind," and certainly contains some very novel and curious statements both about the past and about the future of mankind. He has, however, no very clear notion of the difference between saying a thing and proving it, and many of his most remarkable assertions are made without any serious attempt of proof. His main thesis appears to be that the processes of the inorganic and organic worlds alike are the consequences of a series of quasi-hypnotic "suggestions" on the part of a "great first cause." He takes, that is, a few unfamiliar and very imperfectly understood facts of experience and makes them the basis of a theory of experience as a whole. Apparently he has never even asked himself whether there is any evidence to show that a creature without a nervous system would be amenable to "suggestion" at all. The argument from the miraculous narratives of the Bible, on which he lays great stress, is deplorable alike from the standpoint of logic and of piety. From the logician's point of view, the alleged facts are insufficient as a basis for a theory of nature, and from that of the believer they lose all their moral significance by being degraded to the level of mediumistic or hypnotic "phenomena." A. E. T.

Jahrbuch der Chemie. Herausgegeben von Richard Meyer. Jahrgang, 1900. Pp. xii + 565. (Brunswick: F. Vieweg und Sohn.) Price 15 mk.

THOUGH somewhat later in the time of its appearance, this valuable publication is happily not much thicker than its precursors, and the volume before us gives in reasonable space an excellent summary of the chief advances in chemistry and applied chemistry recorded in the year 1900. The labour of writing is distributed among authorities of the highest competence, and the result is correspondingly satisfactory. It is true that the information is in a highly condensed form, but the present writer is able to say of the subjects on which he is at all qualified to speak that they are dealt with in summaries which bear the impress of informed writers rather than hack abstractors, and that they will continue to serve well the useful purpose of assisting all those who are engaged in the difficult task of keeping themselves moderately well informed of chemical progress.

A. S.

LETTERS TO THE EDITOR.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. No notice is taken of anonymous communications.]

Earthquake Observations in Strassburg.

DURING the last twelve months, on more than one occasion I have been asked why it is that at the Kaiserlichen Hauptstation für Erdbebenforschung in Strassburg one type of instrument records earthquakes so very much more frequently than other types of instruments give records, although they are all installed in the same building. An answer to this is apparently to be found in an analysis of the Strassburg registers.

For example, in January 1901, a von Rebeur-Ehlert apparatus, which consists of three horizontal pendulums oriented at 120° to each other, which reflect beams of light on to a photographic recording surface at a distance of about three metres, yielded twelve records, only five of which were noted by a single component horizontal pendulum of the type adopted by the British Association and now in use at very many stations round the world. This latter apparatus was therefore quiescent on seven occasions when we should have expected it to have been in action. On looking at the registers, we first observe that these seven disturbances were all exceedingly small, and two of them were only noted in Strassburg. Considering this latter fact, in conjunction with the facts that they are found in the traces from an instrument with a very high multiplication, subject to so-called "Mikroseismische Unruhe" (air tremors?), and that a blur may be formed in the photographic record by a slight flare in the illuminating apparatus, it seems a bold proceeding to enter such records (January 17 and 26) as being earthquakes. I doubt their seismic character and consider that their entry ought to have been accompanied by some qualification. So much for two out of the missing seven. Two others (January 8 and 30), although not recorded by the British Association type of instrument in Strassburg, were recorded by similar instruments in Britain and at stations in other parts of the world. That they were not recorded in Strassburg, but were recorded all round Strassburg, suggests the idea that the instrument as installed at the Hauptstation has not the desired amount of sensibility, and if this is the case it is not remarkable that this form of instrument as used in Strassburg should fail to record very small earthquakes.

As another illustration let us take the month of August, when the Rebeur-Ehlert pendulums gave twenty-four records, out of which the British Association seismograph is advertised as only having responded to four. A glance at the registers for stations in Britain and other countries shows that this number should be increased to seventeen, leaving a balance of seven, which, if they all are earthquakes, are for the most part peculiar to Strassburg, and as such have in my own mind a doubtful character.

Another point connected with the Strassburg registers relates to the determination of origins. To identify a seismogram obtained at Strassburg on September 17 at 4.30 a.m. as connected with an earthquake which shook a small portion of the north of Scotland at about 1.25 a.m. on that morning is asking us to believe more than our reason can accept. Even had the Hauptstation been situated in the south of Scotland itself, it is very doubtful whether its horizontal pendulums would have responded to a local shock originated in the northern part of the same country.

JOHN MILNE.

March 3.

Proofs of Euclid I. 5.

SEVERAL writers have lately expressed their opinions in favour of replacing the present proof of this proposition by an alternative proof based on the supposition that the bisector of the vertical angle of the isosceles triangle is drawn, irrespective of the fact that no construction has been given for drawing this bisector. Now there may be some advantage in using a "hypothetical construction" to prove a proposition, where its avoidance necessitates a long and tedious alternative proof. In the present instance the artifice is absolutely unnecessary, as the proof can be simplified in any of the following ways, A being the vertical angle of the isosceles triangle ABC:—

(1) By adopting Euclid's construction of cutting off equal segments AD, AE from the sides and proving as he does that the triangles ABE, ACD are equal in all respects, and then making D, E coincide with A, B respectively. The method of passing to limiting cases is highly instructive, the only question being as to the advisability of trying to introduce it to beginners, except as an experiment.

(2) By drawing a duplicate DEF of the triangle ABC (this operation being only a slight extension of the method of superposition used by Euclid in I. 4), and proving first that $\angle A = \angle E$ and $\angle B = \angle F$, and second that $\angle A = \angle F$ and $\angle B = \angle E$.

(3) By *folding the triangle* so as to bring AB into coincidence with AC. This is *practically* equivalent to bisecting the vertical angle, but it replaces a "hypothetical construction" by an operation which the beginner can easily perform.

The method of folding has many obvious advantages, and much would be gained if beginners could be taught at once to recognise cases in which one half of a figure could be brought into coincidence with the other half by folding. For example, the property that the common chord of two circles is bisected at right angles by the line joining the centres is obvious when it is recognised that one half of the figure is the fold of the other half. The method is, moreover, hardly more artificial than the method of superposition which Euclid himself employs.

It should be noticed that Euclid's proof of I. 4 involves an assumption which I have never seen pointed out, namely, that *two straight lines cannot touch one another*. If this be not assumed, then when the sides DE and AB are brought into coincidence, the sides DF and AC do not necessarily coincide even though they make the same angle with the same straight line and on the same side of it.

G. H. BRYAN.

The Zodiacal Light and Sun Pillars.

THE appearance on clear evenings of the zodiacal light *after* sunset at this season of the year in this latitude is usual, and it has been frequent and beautiful to observe in this district for many nights. It would be interesting if the readers of NATURE could detect any definite movement of the arm of light, for much yet remains to be discovered about this phenomenon, and any observer can make this point a study. From a short half-hour after sunset to from 8 to 9 p.m., a straight line drawn from the sun's position at sunset to the Pleiades will not remain the centre line of the zodiacal light. It appears to emanate from the sun and move as the luminous spoke of a wheel which has the sun for centre frequently, but not invariably. What makes the light apparently fade away? Is it that the motion of the earth has drawn with it the arc of volcanic or meteoric particles, which may be the medium of the light, away from the sun? or is it not possible that such a band of dust is lit with a degree of earthshine? If so, may not this account for that other phenomenon of the Gergersheim, which is usually brilliant in proportion to the brilliancy of the zodiacal light? Doubtless these phenomena are always present, but their visibility depends on the magnetic or electric condition of our atmosphere. Irritated by either of these conditions, the belts of dust would alter positions of all the bodies forming them, and so lie at a different angle and be clearer or dimmer accordingly.

The very remarkable sunset of March 6 has probably been observed by many readers of NATURE. The "fire-finger" left in a perfectly perpendicular position for upwards of fifty minutes after sunset was visibly withdrawn, without losing colour or size or changing from the perpendicular, and was a vivid and beautiful adjunct to a sunset afterglow strangely reminding one of the "Krakatoa sunsets" of years ago. This finger of fire the writer has only observed once before, after a similar-coloured sunset over the estuaries of the Medway and Thames last summer, but London smoke dimmed the effect. This rare appearance seemed on March 6 to resolve or be replaced by five vivid *white* rays with slight wheel motions from north to south, the largest of which eventually seemed the zodiacal light itself.

GUY J. BRIDGES.

Sutton Mandeville Rectory, Salisbury.

A PHENOMENON was visible here this evening which I think deserves to be recorded. We have an uninterrupted view of the western sky, and about a hundred pairs of sharp eyes are available, so that any unusual sunset is pretty certain to be noticed.

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Solar halos are comparatively common occurrences, and I have come to the conclusion that lunar rainbows are not so rare as is believed, but the "pillar of fire" which has been visible here for at least forty minutes is the most brilliant sight I or any of my oldest friends have ever witnessed. It was first observed at 6 p.m. just after the sun had disappeared, and was exactly *vertical* over the sun. The colour was at first silvery (resembling a searchlight) and later a golden yellow, the width equal to the sun's diameter, and the length 18° to 20° . A few light clouds seemed to pass *behind* it. Some observers noted a flickering and also a swaying motion, but this may have been an optical effect. At about 6.30 the colour had changed by gradations into a deep crimson-red, and for the next ten minutes it gradually became deeper in colour and shorter, disappearing at 6.40.

I may add that on February 20, 1901, I observed a very faint trace of a similar phenomenon.

Can any of your readers direct me to any literature bearing on the matter?

WM. A. KNIGHT.

Sexey's Trade School, Bruton, Somerset, March 6.

The Quadrantid Meteors.

MR. JOHN R. HENRY, according to his letters in NATURE of January 2 and 23, unfortunately looked out too late for the Quadrantids, owing to having miscalculated the time of maximum, the approximate probable time of which might also have been obtained from the British Astronomical Association. He is right in saying that the date of the shower is advancing into the year, but the advance is slower than he thinks. Taking the data he gives, which, however, are only very rough, and also a consideration of the sun's longitude as given in the "Nautical Almanac," there is an advance of only five hours in the thirty-seven years 1825 to 1862. It would appear that in the forty years from then a further advance of probably about eight hours has taken place. It is unfortunate that Prof. A. S. Herschel has not published the exact number of meteors he saw from hour to hour on January 2, 1900, when he watched from 11h. to 16h. 30m. He, however, states that the frequency continued about the same during the whole period, and seeing the radiant point was rising all that period, this would mean that the maximum was near the beginning of his watch.

Mr. Henry may be right in saying that the period of maximum fluctuates somewhat from year to year, though the data he gives are not sufficiently accurate to prove this; but I fail to see what ground he had for expecting the maximum so late as he did this year. Taking all the data into consideration, we might have expected the maximum to be about 23h. on the 2nd. I see no reason to doubt that this expectation was fulfilled; but as the maximum would occur in the daytime, observations in other countries would be necessary to prove this. The Quadrantids as seen here were most numerous on that morning.

It seems probable, therefore, that the time of the next maximum will be about 5h. on January 3, 1903.

T. W. BACKHOUSE.

West Hendon House, Sunderland, March 5.

Elementary Mathematics.

I WAS very glad to read in NATURE of January 30 (p. 297) the letter of Mr. J. W. Marshall on elementary mathematics, because all his suggestions referring to elementary algebra have already been realised in my book, "Applied Algebra," published in 1900 at St. Petersburg, in Russian.

I send, therefore, two copies of my book, one for the editor, the other for Mr. Marshall. The algebraical characters will permit every mathematician to judge somewhat of the character of a mathematical book without a knowledge of the Russian language. My compatriots have already condemned my heresy; but I hope that my ex-compatriots (our family is of Scottish origin, derived from Lehighmouth) will judge more liberally my attempt to improve the old method of teaching the prolific science of algebra.

The peculiarities of my exposition are explained by the fact that I wished to say all that was needed, and only what was needed. For that purpose it was necessary to put in the first place the systematical exposition of all the most fundamental methods of algebra which are sufficient for ordinary applications, and to postpone to a supplement all complicated questions that are usually combined with fundamental notions in the existing manuals, only producing a confusion in the minds of the pupils.